*Some Remarks on the Total Solar Eclipse of July* 29, 1878.

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I venture to lay before the Society a few results which I have obtained during the recent total solar eclipse in Colorado, and also some theoretical considerations, which I hope may help in the discussion of other observations.

I was stationed at West Los Animas, the capital of South Colorado. The telescope, with a $4\frac{1}{2}$-inch objective, belonged to Colonel Campbell, of Blytheswood, and my best thanks are due to him for his generosity in placing the instrument at my disposal. The spectroscope had a single prism of dense white flint.

Several hours before the eclipse I covered my left eye, which I intended to use during the eclipse, with a piece of black velvet; all observations before totality were made with my right eye. The enormous extent to which the eye is rendered more sensitive by being kept in the dark is seldom realised. Half-an-hour before the eclipse I uncovered the eye for a few seconds. A sheet of white paper which to my right eye looked a dull white, appeared so dazzling to my left eye that I could not bear to look at it.

I watched the solar spectrum as the Moon gradually began to cover the solar disk, and I was greatly struck with the fact that the violet lines came out so very much more clearly than I ever remembered having seen them under the same circumstances. Although at the time I considered this an apparent effect due to the fact that the general illumination was diminishing and the eye becoming more sensitive, it was so striking that I continued to make notes on the subject until ten minutes before totality, when I could see beyond K. After the end of totality Prof. Eastman told me that he was struck by the great distinctness of the violet lines before totality, and Prof. Young mentions, in the account of his observations, that ten minutes before totality he could, with a fluorescent eye-piece, see dark lines quite to O, and rather better than before the eclipse began. I cannot now consider that this fact, which was so conspicuous as to have struck three independent observers not looking out for it, was simply due to an effect of contrast. In my own instrument, for instance, the general illumination had little to do with the visibility of any lines. It is the intensity of the rays in the blue which weaken the sensitiveness of the eye for the violet, and the intensity of these rays did not decrease as the eclipse proceeded.

I believe I can offer an explanation of the phenomenon. It is well known that under different atmospheric conditions, which even apparently may be alike in every other respect, the

blue end of the solar spectrum is sometimes very vivid, sometimes not visible; and it is easy to imagine that the cooling effect of the eclipse should produce the change from one of these atmospheric conditions to the other. We know, for instance, that aqueous vapour absorbs the violet rays; on the other hand water is, after quartz, the most transparent substance for the ultraviolet. If we assume that the cooling which takes place during the eclipse condenses part of the aqueous vapour, it would follow as a natural consequence that the violet rays should come out more distinctly.

Ten minutes before totality I began to watch the eclipse, in order to see whether the corona was going to be visible before totality. I looked through one tube of an ordinary opera glass; but totality was close at hand before the corona was visible. A chronometer was standing close by me, and I began to count seconds from the time I saw the corona up to the time the last ray of the sun disappeared. I counted six seconds. I then went to my spectroscope, which previously had been pointed to the violet end. My friend, Mr. Haskins, Fellow of St. John's College, Cambridge, was at the finder, and we had previously agreed that unless he saw a large prominence he should point the telescope to the brightest part of the corona. He pointed it to the corona, and I was struck by the intensity of the continuous spectrum. I could see into the violet as far as wave-length 4070, which is further into the violet than the hydrogen line *h*. Widening or narrowing the slit seemed to have no appreciable effect on the limit of the spectrum. I then asked Mr. Haskins to point the telescope to a point further removed from the Moon, and he accordingly pointed it to a part which was three-tenths of the lunar diameter away. I could see no change in the intensity of the spectrum. I next moved the telescope of the spectroscope into the green, and was at first sight startled by not seeing the green line. Looking again at the place, I thought I could discover two faint lines,* but before I could register their position the bright chromospheric lines appeared, and remained for several seconds after the Sun had reappeared.

I saw no dark lines crossing the continuous spectrum. That these dark lines existed I have no doubt, from the observations of Profs. Barker and Morton, who observed in Dr. Henry Draper's party. Yet others beside myself have not seen the dark lines, and I consider this strong evidence that only part, and perhaps only a small part, of the continuous spectrum is due to reflected sunlight.

The polariscope is likely to give us information as to the proportion of scattered light in the corona. I have calculated the amount of polarization due to the scattering of a particle illuminated by a spherical surface. The whole light scattered from such a particle can be divided into two parts. One part

* Prof. Eastman tells me that he suspected a green line more refracted than 1474 K.

is equal to that which would be scattered by the particle if a source of light shining with an intensity

$$\pi \, Sr^2 \cos \omega$$

were placed at the centre of the sphere; the other part is equal to that which would be sent out by the particle itself, if it was luminous and shining with an intensity

$$\frac{2\pi}{3} \, AS \, (2 - 3 \cos \omega + \cos^3 \omega).$$

In these expressions S is the intensity of the light sent out normally by the unit surface of the sphere; $\omega$ is the angle subtended at the particle by a line drawn to the centre of the sphere and a tangent to the sphere; A is the proportion of the light scattered by a particle in any direction if the incident light is polarized in a plane containing the incident and scattered ray; $r$ is the radius of the luminous sphere. If we decompose the scattered light into two components, one polarized in a plane passing through the Sun's centre, the other at right angles to it, the proportion of the second component to the first will be

$$1 - 3 \sin^2 \phi \, \frac{\cos \omega + \cos^2 \omega}{4 + \cos \omega + \cos^2 \omega}.$$

In this expression $\phi$ is the angle subtended at the particle by the Sun's centre and the scattered ray.

It appears from these expressions that close to the surface of the Sun, where $\omega$ is a right angle, the light scattered from a particle is entirely unpolarized; that as the particle is removed from the Sun, the polarization in any direction increases, until finally, when the particle is at an infinite distance, $\omega$ vanishes and the polarization is complete in a direction normal to the incident ray. The polarization therefore ought to increase as we go away from the Sun. I have only taken a single particle into account. As the line of vision directed to the edge of the Sun will pass through particles which are away from the Sun, the light even in this direction will be polarized; but in any case the polarization ought to increase as we go away from the Sun.

These theoretical considerations teach us the amount of polarization in light scattered from a certain fixed number of particles placed at different distances from the Sun. If we could combine with this an accurate measurement of the percentage of polarized light actually sent out by the corona, we should be able to determine the relative number of scattering particles at different distances, and also the proportion of scattered light to the light due to the incandescence of the particles and other causes, such as ordinary reflection. That we should in this way gain most important information will be obvious to everybody.

As a matter of observation the amount of polarization seems first to increase with the distance from the Sun, reach a maximum, and then to decrease rapidly. The maximum of polarization was first observed by Mr. Prazmowski during the

eclipse of July 1860. It was confirmed by Janssen in 1871; and during the same eclipse Mr. Winter took two measurements, one close to the Sun and one a few minutes away from the Sun. The polarization at the latter place was half as much again as close to the limb of the Sun. During the recent eclipse Prof. Arthur Wright took a series of very careful measurements, but only began a few minutes away from the Sun. He observed a rapid decrease as the distance increased. The reason why the polarization close to the Sun is smaller than at a distance of a few minutes is obvious from our theoretical discussion. The fact that a maximum is reached after which the polarization rapidly decreases admits of only one explanation: that the particles begin to be too coarse to polarize the light in the act of scattering.

The exact determination of the point of maximum polarization must in future form one of the most important parts of eclipse observation. A variation of this distance during different eclipses, which will probably be observed, will lead to important conclusions.

There is one more point to which I wish to draw the attention of the Society. It is well known that during the eclipses of the last few years the Sun's corona was found to be approximately symmetrical round an axis which has been designated sometimes as the axis of the Sun, sometimes as the axis of the ecliptic. The two are inclined at an angle which, as seen from the Earth, varies between over 7° 15′ and nothing. If the corona is of cosmic origin, there is no reason why the corona should be drawn out exactly in the ecliptic, though there may be reason that it should be drawn out approximately in the ecliptic. On the other hand, if the corona is a solar atmosphere, the symmetry should be round the axis of the Sun. It would be a matter of interest to mark on drawings and photographs of the corona not only the Sun's equator, but also the ecliptic. The inclination of these two lines during the eclipse of 1871 was more than 7°. A cursory examination of Colonel Tennant's photographs has led me to think that during that eclipse the corona was symmetrical neither exactly round the solar axis nor round the axis of the ecliptic, but nearer the former than the latter. The point which I wish to bring forward, however, is not this symmetry, but a variation from symmetry which is nearly always observed. The corona has generally been found to be drawn out more in one direction than in another diametrically opposed to it, and the remarkable fact is that the eclipses of 1874 and 1875, which happened at an interval of very nearly a year, so that the Sun and Earth were nearly in the same relative position, resembled each other not only in general features, but also in the fact that the corona was developed more towards the north than it was towards the south. That this fact has some meaning I have no doubt, but what this meaning is can only be made out by a careful examination of the outlines of the corona as seen from different relative positions of Sun and Earth.